

With a view to the instruction of students in engineering science, he proposes an abridged way of stating the theoretical principles of Mr. Heppel's method, considering at the same time that Mr. Heppel's more detailed investigation forms the best model for numerical calculation.

He then uses Mr. Heppel's improved form of the "Theorem of the three Moments" to test the accuracy of the formulæ which he obtained in another way, and published in 'A Manual of Civil Engineering,' for the case of a uniform continuous beam with an indefinite number of equal spans, the successive spans being loaded alternately with a uniform fixed load only, and with a uniform travelling load in addition to the fixed load; and he finds the results of the two methods to agree in every respect.

V. "Remarks on the recent Eclipse of the Sun as observed in the United States." By J. N. LOCKYER, F.R.S. Received December 7, 1869.

By the kindness of Professors Winlock, Morton, and Newton, I have been favoured with photographs, and as yet unpublished accounts, of the results of the recent total eclipse of the sun observed in America. I am anxious, therefore, to take the opportunity afforded by the subject being under discussion, to lay a few remarks thus early before the Royal Society.

The points which I hoped might be more especially elucidated by this eclipse were as follows:—

1. Is it possible to differentiate between the chromosphere and the corona?
2. What is the real photographic evidence of the structure of the base of the chromosphere in reference to Mr. W. De La Rue's enlarged photographs of the eclipse of 1860?
3. What is the amount of the obliterating effect of the illumination of our atmosphere on the spectrum of the chromosphere?
4. Is there any cooler hydrogen above the prominences?
5. Can the spectroscope settle the nature of the corona during eclipses?

With regard to 1, the evidence is conclusive. The chromosphere, including a "radiance," as it has been termed by Dr. Gould (the edge of the radiance as photographed being strangely like the edge of the chromosphere in places viewed with the open slit), is not to be confounded with the corona.

On this subject, in a letter to Professor Morton, Dr. B. A. Gould writes:—"An examination of the beautiful photographs made at Burlington and Ottumwa by the sections of your party in charge of Professors Mayer and Haines, and a comparison of them with my sketches of the corona, have led me to the conviction that the radiance around the moon in the pictures made during totality is not the corona at all, but is actually the image of what Lockyer has called the chromosphere.

"This interesting fact is indicated by many different considerations. The directions of maximum radiance do not coincide with those of the great beams of the corona; they remain constant, while the latter were variable.

There is a diameter approximately corresponding to the solar axis, near the extremities of which the radiance upon the photographs is a minimum, whereas the coronal beams in these directions were especially marked during a great part of the total obscuration. The coronal beams stood in no apparent relation to the protuberances, whereas the aureole seen upon the photographs is most marked in their immediate vicinity; indeed the great protuberance, at  $230^{\circ}$  to  $245^{\circ}$ , seems to have formed a southern limit to the radiance on the western side, while a sharp northern limit is seen on all the photographs at about  $350^{\circ}$ , the intermediate arc being thickly studded with protuberances which the moon displayed at the close of totality. The exquisite masses of flocculent light on the following limbs are upon the two sides of that curious prominence at  $93^{\circ}$ , which at first resembled an ear of corn, as you have said, but which, in the later pictures after it had been more occulted, and its southern branch thus rendered more conspicuous, was like a pair of antelope's horns, to which some observers compare it. Whatever of this aureole is shown upon the photographs was occulted or displayed by the lunar motion, precisely as the protuberances were. The variations in the form of the corona, on the other hand, did not seem to be dependent in any degree upon the moon's motion. The singular and elegant structural indication in the special aggregations of light on the eastern side may be of high value in guiding to a further knowledge of the chromosphere. They are manifest in all the photographs by your parties which I have seen, but are especially marked in those of shortest exposure, such as the first one at Ottumwa. In some of the later views they may be detected on the other side of the sun, though less distinct; but the very irregular and jagged outline of the chromosphere, as described by Janssen and Lockyer, is exhibited in perfection."

The second point is also referred to in the same letter. I think the American photographs afford evidence that certain appearances in parts of Mr. De La Rue's photographs, which represent the chromosphere as billowy on its under side, are really due to some action either of the moon's surface or of a possible rare lunar atmosphere; so that it is not desirable to confound these effects with others that might be due to a possible suspension of the chromosphere in a transparent atmosphere, if only a *section* of the chromosphere were photographed.

Dr. Gould writes:—"You will observe that some of the brighter, petal-like flocculi of light have produced apparent indentations in the moon's limb at their base, like those at the bases of the protuberances. These indentations are evidently due to specular reflection from the moon's surface, as I stated to the American Association at Salem last month. Had any doubt existed in my mind previously, it would have been removed by an inspection of the photographs."

Where the chromosphere is so uniform a light that the actinic effect on the plate is pretty nearly equal, the base of the chromosphere is absolutely

continuous in the American photographs; but in the case of some of the larger prominences, notably those at +146 (Young) and -130 (Young), there are strong apparent indents on the moon's limb.

I next come to the obliterating effect of the illumination of our atmosphere on the spectrum of the chromosphere.

This is considerable; in fact the evidences of it are very much stronger than one could have wished, but hardly more decided than I had anticipated. Professor Winlock's evidence on this point, in a letter to myself, is as follows:—"I examined the principal protuberance before, during, and after totality. I saw three lines (C, near D and F) before and after totality and eleven during totality; *eight were instantly extinguished on the first appearance of sunlight.*"

This effect was observed with two flint prisms and 7 inches aperture. Professor Young, with five prisms of 45° and 4 inches aperture, found the same result in the part of the spectrum he was examining at the end of the totality.

He writes: "I had just completed the measurements of 2602, when the totality ended. *This line disappeared instantly*, but 2796 [the hydrogen line near G] was nearly a minute in resuming its usual faintness." These observations I consider among the most important ones made during the eclipse; for they show most unmistakably that, as I have already reported to the Secretary of the Government-Grant Committee, the new method to be employed under the best conditions must be used with large apertures and large dispersion.

On the fourth point the evidence is but negative only, and therefore in favour of the view I have some time ago communicated to the Royal Society.

We next come to the question of the corona,—a question which has been made more difficult than ever (in appearance only I think) by the American observations.

I propose to discuss only the spectroscopic observations of Professors Young and Pickering in connexion with Dr. Gould's before quoted remarks.

Professor Pickering, with an ordinary chemical spectroscope merely directed to the sun's place during totality, obtained the combined spectrum of the protuberances and corona. He saw a continuous spectrum with two or three bright lines, one "near E," and a second "near C."

Professor Young, who used a spectroscope specially adapted for the work, in which only one part of the prominence at +146° was being examined, saw C, near D, a line at  $1250 \pm 20$ , and another at  $1350 \pm 20$  of Kirchhoff's scale. The rest of the observations I give in his own words.

"Then came the 1474 K line, which was very bright, though by no means equal to C and D<sub>3</sub>; but attention was immediately arrested by the fact that, unlike them, it extended clean across the spectrum; and on moving the slit away from the protuberances, it persisted, while D<sub>3</sub>, visibly in the

edge of the field, disappeared. Thus it was evident that this line\* belonged not to the spectrum of the protuberance, but to that of the corona. My impression, but I do not feel at all sure of it, is that the two faint lines between it and  $D_3$  behaved in the same manner, and are also corona lines†.

"I am confirmed in this opinion by Professor Pickering's observation. He used a single-prism spectroscope, with the slit of the collimator simply directed to the sun, and having no lens in front of it. With this arrangement he saw only three or four bright lines, the brightest near E (1474). Now this is exactly what ought to occur if that line really belongs to the corona, which, from its great extent, furnished to his instrument a far greater quantity of light than the prominences.

"By this time the moon had advanced so far that it became necessary to shift the slit to the great prominence on the opposite side of the sun. While my assistant was doing this, I suppose I must, in the excitement of the moment, have run my eye-piece over the region of the magnesium lines (*b*), and thrown them out of the field before he had brought anything upon the slit. At any rate I saw nothing of these lines, which were evident enough to several other observers, and can think of no other way to account for their having escaped me. The F line in the spectrum of the great protuberance was absolutely glorious, broad at the base and tapering upwards, crookedly as Lockyer has before often observed. Next appeared a new line, about as bright as 1474 at  $2602=2$  of Kirchhoff's scale. Its position was carefully determined by micrometrical reference to the next line, 2796 K (hydrogen  $\gamma$ ), which was very bright; *h* was also seen, very clear, but hardly brilliant. In all I saw nine bright lines.

\* "On two or three occasions previously I had been very much surprised at not being able to detect this line in the spectrum of unusually bright prominences. On the other hand, I once found it very easy to see at a place on the sun's limb where the other chromosphere lines, usually far more brilliant, were almost invisible."

† "A careful examination of the photographs, especially No. 2 of the Burlington totality pictures, somewhat diminishes my confidence in the conclusion of the text as to the nature of these three lines (1250, 1350, and 1474). They certainly do not belong to the spectrum of the most brilliant portion of the prominences; but around the prominences of the eastern limb, on which the slit of the spectroscope was directed during the first half of the totality, the photograph shows a pretty extensive and well-defined nebulosity, evidently distinct from, though associated with, the brilliant nuclei. Now it is possible that these lines may belong to this nebulosity, and not to the corona proper; for I cannot recall with certainty whether 1474 retained its brilliance at any considerable distance from the prominences, or only in their immediate neighbourhood. My strong impression, however, is that the former was the case, and that the text is correct. I may as well confess that my uncertain memory here is due to the fact that just at this time, while my assistant was handing me the lantern with which to read the micrometer-head, I looked over my shoulder for an instant, and beheld the most beautiful and impressive spectacle upon which my eyes have ever rested. It could not have been for five seconds; but the effect was so overwhelming as to drive away all certain recollection of what I have just seen. What I have recorded I recall from my notes taken down by my assistant."

“A faint continuous spectrum, without any traces of dark lines in it, was also visible, evidently due to the corona. Its light, tested by a tourmaline applied next to the eye, proved to be very strongly polarized in a plane passing through the centre of the sun. I am not sure, however, but that this polarization, as suggested by Prof. Pickering, may have been produced by the successive refractions through the prisms. This explanation at once removes the difficulty otherwise arising from the absence of dark lines.”

I have first to do with the continuous spectrum, deduced from Professor Pickering's observations.

I think in such a method of observation, even if the corona were terrestrial and gave a dark line spectrum, the lines visible with such a dim light would in great part be obliterated by the corresponding bright lines given out by the long arc of chromosphere visible, to say nothing of the prominences, in which it would be strange if C, D, E, *b*, F, and many other lines were not reversed. This suggestion, I think, is strengthened by the statement that two bright lines were seen “near C” and “near E;” should we not rather read (for the “near” shows that we are only dealing with approximations) C and F, which is exactly what we might expect.

But even this is not all that may be hazarded on the subject of the continuous spectrum, which was also seen by Professor Young under different conditions.

Assuming the corona to be an atmospheric effect merely, as I have before asserted it to be, it seems to me that its spectrum should be continuous, or nearly so; for is it not as much due to the light of the prominences as to the light of the photosphere, which, it may be said roughly, are complementary to each other?

With regard to the aurora theory, I gather from Professor Young's note that, if not already withdrawn, he is anxious to wait till the next eclipse for further facts. I consider that the fact that I often see the line at 1474, and often do not, is fatal to it, as it should be constantly visible on the proposed hypothesis. The observation of iron-vapour, as I hold it to be at this elevation, is of extreme value, coupled with its simple spectrum, *seen during an eclipse*, as it entirely confirms my observations made at a lower level in the case not only of iron but of magnesium.

February 3, 1870.

Lieut.-General Sir EDWARD SABINE, K.C.B., President, in  
the Chair.

Among the Presents received was a Thermometer, presented by Mr. Augustus De Morgan, which had been made in Florence in the seventeenth century. It was one of a collection discovered in the Museo Fisico of